**Review Round 2: Organic matter composition and origin differ between restored and natural UK saltmarshes up to 100 years after the breach**

Thank you for the opportunity to review the revised submission of "Organic matter composition and origin differ between restored and natural UK saltmarshes up to 100 years after the breach". Firstly, I would like to commend the authors again for making valuable comparisons and collating data across extremely heterogenous environments. Few papers investigating MR schemes encompass the level of site variability examined here, therefore providing an important contribution to the literature regarding the overall effectiveness of MR sites as C stores. Secondly, the authors efforts to amend the paper since last submission is evident and have significantly improved the quality of the manuscript.

My critiques of the revised submission will follow the same formatting as last time. I have. Provided my three most overarching critiques followed by line-by-line comments, referring to the tracked changes manuscript. I have not spent considerable time regarding grammar, spelling, or errors within the references.

1. My primary concern is the methodological consistency in which breach depth is identified. The results presented here have demonstrated that variability within and across sites requires additional proxies alongside foraminiferal data and that determination on a case-by-case basis may be necessary. However, the criteria for selecting a proxy to aid breach identification and in some cases explanation of how these proxies changed breach depth estimates are not clearly defined, which is essential for reproducibility. For example, OC/N ratios were used to widen breach depth identification, but no systematic application was described. Further within foraminiferal data, different criteria seem to be applied, with some breach depths identified based on the presence or absence of foraminifera and others based on changes in their abundance. It would be helpful to have a more developed and articulated framework used across all cores so that the approach can be more easily followed by the reader.

   The additional discussion regarding the benefits and downfalls of breach identification using foraminifera are a welcomed addition to the paper. However, I would encourage the authors to consider changing the use of the word success in certain parts of the paper, considering the method performs well in some cases but varies substantially among sites and cores.

2. The discussion would benefit from a clearer overarching narrative, as it stands the individual sections currently read somewhat independently of one another. The discussion often shifts between broader implications, methodological considerations, and site-specific differences without a clear structure linking these themes together. As a result, the overall narrative can be difficult to follow.

3. The implications of the study's findings within the conclusions should be discussed in more detail. Also, the novelty of this research should be further emphasized. Within the last round of reviews, we had described that reframing the novelty of using foraminiferal dating, of which the authors addressed in this resubmission. However, the broader significance and contributions of this paper are understated.

**Abstract:**
19-24: While C crediting establishes the context of this work, the paper only briefly discusses C crediting beyond the introduction and may place too much emphasis on C crediting within the abstract.

**Introduction:**
107: Rather than having one small paragraph (121-127), it would flow better to make a new paragraph at this line.

112: Change "only" to largely. Recalcitrant and refractory OM can be lost at higher temperatures. Black C has been described to be lost within the thermal regions identified for labile OM (Helfrich et al., 2010).

114-116: This sentence feels somewhat disconnected from the rest of the paragraph, as it introduces biological controls on OM/OC stabilization, whereas the subsequent discussion focuses primarily on physical and chemical stabilization mechanisms and their associated metrics. It may be helpful to either integrate the biological component more fully or clarify its relevance to the discussion that follows.

126-127: It would strengthen the paragraph if this line was omitted or specific explanations were given to the limitations.

132: Is there any reason as to why visual interpretation using stratigraphy was not applied to determine breach depth? Would it be the same reasoning that applying radiometric dating can not always be applied?

**Study sites:**
179-193: Because this section is not site specific and is largely background information, I would suggest this section be incorporated into the introduction.

Figure 1b: The labels in panel b are still difficult to read because of the size. It might improve readability if larger text connected by arrows or lines to the pins. I will note however that changes since last submission have improved the figure substantially.

197: I understand that "restored" indicates that Aldboro Point was not intentionally made to be a restoration project. However, I find it slightly confusing as it could be interpreted as the site may or may not be recovering. I have seen other papers use the word unmanaged realignment, which still differentiates it from the other intentionally managed sites.

209, 227, 259, & 271: These sentences could be replaced with a single sentence saying the distance between coring location and the breach is recorded in Table S1 for each site.

**Methodology:**
292: Should this read that 3 replicate narrow-diameter cores were taken within the same area? If not, it is slightly confusing when considering the other three narrow cores on the map.

395-401: Please report the rate of temperature increase.

403: Why were the narrow and wide cores interpreted separately?

411: See main point number 1. Why was the additional OC and N data considered in this case to extend breach estimates but not for other cores? How was this systematically determined and how was it applied?

413: Please specify what the "totals" refers to.

**Results:**
Figure 3: The simplification of the figure since the first submission has improved clarity. It would substantially improve cohesion of the paper if the categories (red, blue, and grey boxes) were the same across all graphs, otherwise the categories appear subjective.

Figure 4: See main point number 1 and comments about line 411.

Figure 6: The shortened axes have improved the figure. However, the figure is still incredibly complex. Reducing the reference data points to only a few key ones would reduce clutter and highlight the data. Also, the inset plots are too small to see clearly and obscure the graph. I would either make two separate figures, reincorporate the shading to indicate depth or put the depth profiles in the appendix.

669-672: It is not clear if the same breach depth identification methods calculated from the wide cores were applied to the narrow cores.

Table1 header: "using the evidence accumulated during the study" is not clear. This could be clarified to state stock calculations calculated using previously identified breach depths. Further, it was stated in line 667 that breach depth was changed with additional information which contradicts the header which states that calculations are based on foraminiferal results alone.

Table1: The "Cores used for estimate" column is quite text heavy and could benefit from simplification. Also, using the sites full name in the "Site" column could make the table easier to read.

**Discussion:**
715: It would benefit the discussion to provide a few sentences here restating the aims of the paper as well as stating the key findings and novelty of the paper.

789: It may benefit the reader to have the key findings about the breach identification first followed by specific site descriptions and caveats

806-809: If I understand correctly, a 50 cm core with 20 cm of post-breach sediment accumulation should still contain the breach horizon. The discussion in the following lines about spatial variability in accretion rates across the MR site makes sense but this line is somewhat confusing in that context.

817-821: When it is stated that absent foraminifera are interspersed with low abundances, I interpreted this as meaning that the breach horizon is present but cannot be identified with confidence. However, it is then suggested that deeper cores are needed to capture the pre-breach agricultural sediments. These seem like two different issues, and it is not clear how they are linked.

1041-1045: This is more of a justification of the methods used and does not particularly add to the implications of the findings. I would suggest moving this sentence to the results or methods section.

1077: Because this section is largely surrounding OM composition, renaming it could make the structure and progression of ideas easier for the reader to follow.

1105-1119: Perhaps this belongs within the breach identification section.

**Conclusion:**
1168-1179: This paragraph provides a succinct overview of the study. Moving it to the beginning of the Discussion, or adding a similar brief overview there, could help orient the reader.

1180-1188: The implications of the findings are clearly articulated here. However, the implications are not discussed in as much detail throughout the rest of the Discussion. It would strengthen the paper to integrate more of the implications and broader meaning of the results throughout the Discussion or alternatively expand this section further. For example, while the Introduction provides a detailed discussion of C crediting, only a single sentence is devoted to this topic here, leaving an opportunity to more fully connect the findings to wider applications.

1158-1202: providing a brief statement of the contributions or novelty of the paper would bolster this works contribution to the current literature.

**Supplemental:**
Figure S4: Please include what is defined as low number of tests in figure legend and/or the key.

Overall, the edits made since last submission have significantly improved the paper. The results of this study demonstrate that the interpretation of results across different types of MR sites is imperative to consider due to the substantial variability within and across MR schemes. This

study therefore offers an important contribution to the literature as it collates data across multiple sites, which is often a limiting factor in many other studies. The aforementioned comments detail that the paper would greatly benefit from for a clearer and consistent description of the approaches used to identify breach depth, as well as a clearer progression of ideas in the discussion with a greater emphasis on the implications of the findings. I enjoyed reading this paper and hope that the authors find these comments constructive.